

# TRUST ME, I'M A DOCTOR: A PHD SURVIVAL GUIDE

*Short title for running header:* A PhD Survival Guide

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**Abstract:** So, you've decided to do a PhD... now what? In this essay, the author provides some advice for beginning PhD students—basically sharing what he would tell his younger self. Doing a PhD is a transformative experience, but the process is challenging—not merely on an intellectual level but also psychologically. To overcome these challenges one needs a certain mindset and a bag of tricks. The author offers some help for getting in the right mindset, and shares some of his own tricks for studying, research, and productivity in general.

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If everything goes well, I should have a PhD by the time you are reading this essay. That means I have a good collection of deadlines at the moment, so I should definitely not be wasting time on writing an essay that is not strictly necessary. But the urge to procrastinate is strong, and of all the ways in which I could give in to the temptation, this seems the most acceptable one.

In this essay I want to share some autobiographical anecdotes and some insights or bits of advice. My goal is basically to tell you what I wish someone had told me five years ago.

But before we get started: Why am I writing this survival guide? By now, I've seen several new cohorts of first-years start a PhD. These people are always highly motivated and very smart. Unfortunately, being motivated and smart is not enough to survive. Everyone in academia knows some very smart people who started, but never finished, their PhD. People drop out because they feel that they are not up to the task. The tragedy is that this perception is often wrong. They have the skills and the intelligence; what is lacking is perhaps a good understanding of what a PhD is, and a bag of tricks to overcome the obstacles along the way.

Practically everyone—*definitely* including myself—has had serious self-doubts at some point in the PhD and has considered quitting. I don't want to scare first-years, but I've felt inadequate, stupid and/or unfit for the job most of the time. It has only started to feel “natural” in the past few months.

Like many other people, I found doing a PhD very hard—definitely the most challenging thing I've ever done. It's difficult, but not in the same way in which a hard mathematical problem is difficult. The biggest challenges were psychological, and I suspect that it's these psychological challenges, and not the intellectual ones, that are most stressful to PhD students and the biggest cause of people giving up. However, with the right mindset and some tricks, you can face these challenges, as I discovered along the way. The purpose of this essay is to tell you

about those two things: the mindset and the tricks, which I'll tell you about after I discuss the challenges of a PhD.

## **THE CHALLENGES OF DOING A PHD**

Let's start with the good news: If you got hired, you are smart enough to do a PhD. Now for the bad news: Being smart is necessary, but not sufficient.

Being smart means you are good in finding an answer to a question. In a PhD, however, you often don't really know what the question is, let alone if there is an answer at all. This is not an easy feeling for your brain, and it's easy to feel discouraged.

Your education up to this point has not prepared you for a PhD at all. Studying textbooks and solving problem sets is quite easy compared to trying to figure out how the hell you can make sense of the empirical results you found, or trying to figure out how you can abstract a difficult problem into a tractable theoretical model without losing the essential aspects of the problem.

As a result, during the PhD, you will quite often feel stupid. This is normal and even expected. The biologist Martin Schwartz wrote a short but brilliant essay called "The Importance of Stupidity in Scientific Research," which I strongly recommend. He makes the point that

if we don't feel stupid it means we're not really trying. I'm not talking about 'relative stupidity', in which the other students in the class actually read the material, think about it and ace the exam, whereas you don't. I'm also not talking about bright people who might be working in areas that don't match their talents. Science involves confronting our 'absolute stupidity'. That kind of stupidity is an existential fact, inherent in our efforts to push our way into the unknown. (Schwartz 2008).

I put his essay up on the door of my office and I've read it over and over again—my first concrete advice to you would be to read his essay.

Many people starting together with you in the PhD feel equally insecure, and don't want to admit they are having difficulties and doubts as well. Of course, if nobody admits that they have a hard time understanding econometrics or that they have serious doubts about their capacities, you will have the impression that all the others must be geniuses—that everyone in your PhD program is Einstein, except for you. In reality, however, they are all worrying about the same things. The best strategy is to become friends, share your doubts and worries, encourage each other and try to solve problems together. It makes the whole process a lot less stressful and a lot more fun.

Psychologists have described a related phenomenon known as “impostor syndrome.” This is when despite your achievements you have the feeling you are actually a fraud, that all your accomplishments were just good luck, and that any day now, people will find out that you're not really that smart. You would be surprised at how many PhD students experience this. In my first year, I constantly felt like I was surrounded by all these smart people and that I would fail the exams and get fired. Even after the first semester, in which I had excellent grades, I was semi-convinced that this was due to luck and that my grades would revert to their true value in the second semester. And then, after the second semester had also gone well, I told myself that taking courses was quite easy compared to doing research, at which I was clearly not good, and so on.<sup>1</sup>

A first step to overcoming “impostor syndrome” is to realize that it's a well-known psychological phenomenon, a bug of the mind, like an optical illusion. A second step is to tell

yourself that, even if perhaps your previous accomplishments were due to luck, it is possible to grow and improve yourself to match the expectations. That's what I want to talk about next.

### **YOUR MINDSET: FIXED OR GROWTH?**

There are, roughly speaking, two big “mindsets” people can have when they are trying to learn a new skill. These mindsets are usually subconscious, but they influence your motivation and how you think about failure.

The *fixed* mindset is the belief that you were born with a fixed set of skills and competencies, and that some activities or skills are just not “your thing.” It corresponds to the belief that success is determined by natural talents such as your IQ. With this basic outlook on life, if you try something and fail, you will interpret it as a signal that this must not be one of the activities you're naturally good at. Failure in an activity is a signal that “this is not for me.” Because of this interpretation, you're not very likely to continue practicing. Why bother trying again, if you were not made for this?

The *growth* mindset, by contrast, starts from the assumption that being good at something comes down to a little bit of skill and a lot of hard work. Sure, knowing how to play an instrument depends on a bit of musical talent. But given a minimum of musical talent, you just need to practice like crazy before you can play the piano or the clarinet reasonably. With this mindset, failure is not a signal that you should stop whatever you were doing. On the contrary, it is a signal that you need more practice. With a growth mindset, you would analyse your failure in more detail to figure out what went wrong and why, so you know what to focus on in the future.

Many people, including myself, have the fixed mindset as a “default” setting. If you already have a growth mindset, all the better. If not, there are three points I want to make.

First, know that the mindset can be changed. This is perhaps the most difficult step: If you believe that success in life comes down to innate skills or talents, it seems likely you will also believe that your mindset is given! But it's not. To take one dramatic example to illustrate this point, people suffering from depression seem to have a view of the world that is similar to the fixed mindset, which leads to a "negative explanatory style." Depressed people tend to think the causes of failure are stable (fixed), global (it affects everything they do) and internal (they are the root of the problem). Therapy often focuses on tackling this explanatory style, making patients aware of these thought processes and helping them to consciously change them. Explanatory style can be changed.<sup>2</sup> If people with severe depression can overcome their negative explanatory style, then surely you can overcome your fixed mindset.

Second, there are good reasons to believe that in reality your innate talents matter less than your effort, so that the growth mindset is actually the correct way to think about things. The media like to portray successful people as people who just happen to have enormous innate talents. In movies, a smart person is a "genius" who on the spot has all the brilliant insights necessary to solve a problem. Movies never show the smart person as someone who spent years carefully studying a topic and gradually realizing how the puzzle fits together. But it's this second version that is more accurate. People who are successful in a field have usually worked *incredibly* hard to get where they are. It appears that experts in any kind of field often spent around 10,000 hours perfecting their skill.<sup>3</sup>

Third, switching to a growth mindset will make you happier anyway. With a fixed mindset, failure is a hard, damning, eternal judgement on your talents. You try to play the piano and fail? Clearly, you have no musical talent whatsoever and you'll never be able to do it. Now, contrast this with the growth mindset. You fail on the piano? You could probably master it if you

put in enough deliberate practice. There's nothing wrong with you, just practice a bit more. Or don't, if you don't really like the piano. It's your choice.

Now, while it's possible to override your fixed mindset, it's highly likely that it will remain your "default" setting. I still have to fight it. For instance, it was quite a psychological barrier back when I was learning how to drive a car (which is embarrassingly recent). There was a constant struggle between the "default" of my mind telling me I'd never be good at it, and the conscious realization that if so many idiots can drive a car, surely I should be able to learn it too, if I only practiced enough. And it was true!

Even if you accept that it would take hard work to get where you want to be, you will not always have sufficient energy or motivation to actually put in the work. You may say: "I know I should study, but I simply can't find the energy; I'm simply lazy." First, you are not *simply* lazy—that would again be a form of fixed mindset thinking. Everyone feels unmotivated now and then, and there are a number of factors which typically drain motivation (e.g., a lack of deadlines; a lack of clarity about what exactly needs to be done). Moreover, even the most disciplined people I know complain that they are terribly undisciplined; it's normal that your energy levels are not always 100 percent.<sup>4</sup>

How does all of this relate to your PhD? With a fixed mindset, you're in for a great deal of stress. If you try to figure out some statistical technique or mathematical problem but you fail, your mind will interpret this as yet another sign you're not really smart enough to be doing a PhD. You'll feel like you're not supposed to be in the PhD program (the "impostor syndrome" mentioned earlier). Because a PhD is challenging, it's almost guaranteed that you will encounter failure, so you will have lots to stress about, and perhaps you'll even convince yourself that you shouldn't be in the PhD program at all.

With a growth mindset, you realize failure for what it is: a signal that you need more practice. If you don't understand this or that technique or problem, you should realize that many other people in the field who are now experts also had difficulties understanding the issues in the beginning, and that it's just a matter of finding the right way of approaching the issue. This takes away a lot of stress. Instead of thinking "I can't figure out how X works, I must be too stupid for this job," you would now say "I can't figure out how X works—what would be a good strategy to crack this problem?" This is a lot more productive.

### **DEALING WITH REJECTION**

A growth mindset will also help you survive setbacks and criticism. And you will have your fair share of those. When I was in my first year, I applied for a scholarship from FWO (the Flemish science foundation). The rejection letter said, among other things, that I had been "good, but not excellent" as a student. The memory still makes me angry. I've always thought of myself as having a comparative advantage in intellectual matters, and quite a bit of my self-image depends on the feeling that I'm good at those things. Receiving an official letter from a government agency dissing my track record was not much fun, especially because I was still in the "impostor syndrome" phase I talked about earlier. So, I shook my fist at the skies while yelling "FWO!". Better luck next year, I thought.

Alas, no. The next year I spent a lot of time writing what I still think was a pretty cool research proposal (about using games to measure social preferences among fishermen on a lake in Benin), only to receive a letter saying that although my research proposal was very important and very creative too, unfortunately it should have been more detailed on the methodology part. (There was a limit of three pages to write the entire proposal—introduction, literature survey, research question, methodology, how the proposal fits in the research of your group, and



references). Because you can only apply for FWO twice, that was game over. Fortunately, I could still apply for travel grants!

Except those got rejected, too—in one case because, as the official explanation put it, “we ranked all the requests and yours did not make it,” which was very helpful feedback indeed. Nowadays, I can joke about these rejections, although I still have revenge fantasies about that “good, but not excellent” letter. Back when I got my first rejection letter, it must have been the first time I truly failed something academic, and I was shocked. The second rejection felt equally bad, because many knowledgeable people said I had a good chance of getting the funding. When I got the news, I was at a conference with my thesis advisor, and he saw that my motivation was pretty low after this second rejection. Over a beer, he told me that the correct response in this case was to tell myself that I would prove them wrong. I agree that’s the right way to think about it.

One thing which helped me deal with rejections was realizing that rejection is standard, and that many good economists have had their work criticized and rejected.<sup>5</sup> For instance, George Akerlof’s classic “The Market for Lemons” got rejected at three journals before finally being accepted at the *Quarterly Journal of Economics*! Even top economists are routinely rejected, so you shouldn’t feel too bad about it.

### **A BAG OF TRICKS**

You might think that as a PhD student it is expected of you to simply *know* how to study, how to do research, and so on. At least, nobody will tell you how to do it. And so we spend our time teaching students about obscure statistical techniques but not about the more frequent obstacles they will face, like trying to get your goddamn data in the correct format so that your statistical software can read it properly. What I discovered is that studying and researching and all other

things related to a PhD—and probably to any career—depend on little tricks and techniques, and “experts” in a field are often people who also happen to know a lot of these little tricks.

Because I don’t know all the tricks myself, and because the tricks you need might depend on your personal characteristics, the following is not absolutely exhaustive. So, when you get stuck on something, try to find out which trick would help you solve the issue. A good trick to find tricks would be to ask people who are very good at something—they probably figured out the essential tricks through practice and experience. Talk to them!

Most of the tricks you need are psychological. Yes, we’re supposed to be super-smart scientists here, but in the end we are just normal people, with all the normal psychological weaknesses (cognitive biases, procrastination, absent-mindedness, lack of focus, and so on). Fortunately, in recent decades there’s been an explosion of work on behavioral economics and on psychological tricks to overcome these weaknesses. If you have time, I’d definitely recommend that you read a bit of behavioral economics. It’s fascinating and useful, what more could you want? A good starting point is *Nudge: Improving Decisions about Health, Wealth and Happiness* by Thaler and Sunstein (2009) or *Thinking, Fast and Slow* by Kahneman (2011).

I almost didn’t notice, but I just applied a trick here: If you want to know more about a subject, try to read a popular science book or article instead of diving straight into the academic literature. “But, Koen,” you might be saying, “I don’t have time to read those popular science books!” Ah, but if you can find a good popular science book about a topic you are working on, it will actually save you a lot of time down the road. You’d be more knowledgeable, it would be easier to understand arguments in the literature, and it would be easier to see to which other studies your own work can be linked. Of course, not every field or topic has good popular

science books but you would be surprised at how many good popular science books are out there, even about topics such as statistics or game theory.

If you are looking for tricks for organizing your time, getting things done, learning more, reading faster, or whatever you would like to accomplish, there are tons of books and blogs that can help you. If you are struggling with something, probably one of the other seven billion people on this planet has had a similar problem and you might be able to learn from their experiences. Some resources I found particularly helpful:

- Josh Kaufman’s Web site ([joshkaufman.net](http://joshkaufman.net)) and book *The Personal MBA: Master the Art of Business* (2012) as well as his list of 99 recommended business books. Check it out even if you are not interested in running a company—the book and the reading list cover topics such as “The Human Mind,” “Working with Yourself,” and “Working with Others,” all of which are useful for academics.
- David Allen’s book *Getting Things Done* (2001), a classic on how to avoid being overwhelmed by to-do lists. I probably only apply about 10 percent of his advice, but it has already made a huge difference.
- Cal Newport’s blog *Study Hacks*. Newport is an academic researcher himself, and he emphasizes the importance of deep focus and avoiding distractions. This conflicts with David Allen’s focus on executing to-do lists (which Newport once referred to as “getting unremarkable things done”), but both have important and useful things to say.

Below I present some more tricks. Needless to say, this is not the bible; experiment with the tricks and see what works for you, and exchange tricks with others.

### **Tricks for Studying: Connections**

I think the best strategy when you're studying anything is to focus on understanding first, and memorization later (and only if necessary). What does "understanding" mean? I have a theory that nobody really understands anything—we just recognize that some things work like other things we are already familiar with.<sup>6</sup> Sometimes "understanding" means finding the exact (mathematical) connection between something we don't understand and something we do understand. Other times, it's more a matter of metaphors or similarities.

The way I see it, "knowledge" or "understanding" is like a giant web of ideas in your head. Learning is a process of creating new connections between those ideas, or adding new ideas by linking them to existing ones. The implication for learning is this: Don't try to memorize things immediately; instead, try to first create connections in your head.<sup>7</sup> In the longer version of this essay (Deconinck 2014), you'll find some tricks to help you make those connections by making exercises, finding a story, finding silly connections, explaining the material to somebody else, using extreme examples, and—again—finding allies among your fellow PhD students.

### **Tricks for Research**

I don't think I'm a good researcher at all, but I do believe I've improved a lot compared to five years ago (thank God). As I said earlier, research is incredibly hard. If you're making a difficult exam, at least the question is more or less clear and, because the question is being asked on the exam, there is probably an answer. In research, often the question is not really clear, and it's absolutely not clear if there is an answer at all. Moreover, the question could turn out to be the wrong question completely. You could fail to find an answer, or you could come up with an answer to a different question, and you might not even realize it. Research can be a bit complicated. Here are some ideas which will hopefully be useful to you.

*You Are a Detective, Not a Statistics Robot*

After finishing my coursework, my first assignment was to write a book chapter about beer consumption in Russia (Deconinck and Swinnen 2011). My thesis advisor told me beer consumption had increased dramatically in Russia over the past 15 years and asked me to figure out what was going on.

Now, because I had just finished the doctoral program, my math and econometrics skills were better than ever. But none of it prepared me for that paper. My thesis advisor had told me to look up beer consumption on the FAO Web site, which was easy enough to do. But how do you go about finding “the cause” of the rapid increase in beer consumption? What data are you looking for? Where can you find it? I got stuck pretty quickly.

The problem is that most of the doctoral program (at my university and elsewhere) seems built on the assumption that research is almost a routine job: pick hypothesis, build model, collect data, test model; repeat. We spend most of the time on building the model or testing the model, but almost zero time on picking the hypothesis or collecting the data.

When you’re picking a hypothesis, which one should you pick? Out of the bazillion possible hypotheses of what caused beer consumption in Russia, which one is most plausible or most interesting to look into? It’s a little known fact of science that there is an infinite number of hypotheses out there, and you could spend a lifetime checking all of them if you were blindly following the “routine” model (and almost none of them would give you any interesting results, let alone a publishable paper).

Collecting the data is another step which is usually glossed over. Surprisingly, not all the information in the world is already available in an Excel file for you to import into Stata. Even

more surprisingly, a lot of data is not quantitative. I had to become a lot more flexible in thinking about “data.”

In the Russian case, I realized I needed to stop thinking like a statistics robot and start thinking like a detective, finding information wherever I could. For instance, I wrote in the chapter that beer in the Soviet era was apparently pretty awful. I based this assessment on newspaper reports and on the fact that when foreign investors entered the Russian market they spent millions to upgrade the breweries to produce higher quality beer. It’s a judgment call, because you could argue that I don’t have enough data to make this statement; you could insist I should find objective sources on beer quality around the world during the Soviet era (good luck with that). However, people familiar with the situation confirmed that quality was poor, and available information points in the same direction, so we can be more or less confident that Soviet beer was not exactly a pleasure for the taste buds. This is relevant information if you’re trying to find out why beer consumption changed, but it’s not the kind of information I was originally trained to look for.

The bottom line is that “data” is not necessarily the hard numbers you’ve been assuming throughout your education, numbers which seem to fall from the sky as objective representations of the truth (in handy .csv or .dta format). Instead, think like a detective. Perhaps you don’t have data on the thing you’re studying, but you have indirect information which can be used as a proxy. For instance, we don’t always have good income data for developing countries, but we can use ownership of assets such as cars, houses, bikes or TVs to construct a “wealth” proxy. Be creative but, like a police detective, keep in mind that your evidence needs to be sufficiently powerful to convince the jury.

*Keep It Simple*

Again, much of our education focuses on fancy techniques (Generalized Method of Moments! Infinite-horizon dynamic stochastic programming!), but it's more important to first get the story straight. And the story can usually be told without integral signs.

Suppose you are looking at beer consumption in Russia, again, and your hypothesis is that beer prices changed, leading to the increase. That would be a plausible hypothesis. But, being a good economist, you know that prices are not really exogenous. So, you start thinking of ingenious instruments to study the effect of beer prices on beer consumption.

But hold on a second—your data on beer prices does not really show any big changes, while beer consumption quintupled. So, first of all: Because the change in beer consumption is really large, we would need a very big change in beer prices or a very price-sensitive demand for beer for this story to work. And second, your beer prices don't seem to have changed all that much. Perhaps they contributed a little bit, but do we really believe that a small decline in beer prices led to a quintupling of beer consumption over the span of ten years or so? That doesn't seem plausible at all. So, forget about your search for an instrumental variable for beer prices: It's probably not worth the trouble.

You can see the problem here. If you start off with the technique, you can easily spend months perfecting and implementing the technique only to discover that there's nothing going on. And quite often, you could have known up-front that you wouldn't find anything.

If you are getting started on an empirical paper, and your hypothesis is that A causes B, try a simple scatterplot first. Or make a graph in Excel to see whether A and B move together over time. Then, do a simple regression with a few extra variables to see if there's anything going on. This could save you months of hard work and frustration. Moreover, some of the most

impressive papers in economics use little more than OLS or instrumental variables to make their point. Fancy techniques are often not necessary to tell a compelling story.<sup>8</sup>

*Discover the Wisdom of György Polya*

The Hungarian-born mathematician György Polya (1887–1985) wrote a little book called *How to Solve It* (1945) with advice on how to tackle mathematical problems. On one of the first pages of the book, Polya put an overview of the approach, which I've copied and put up in my office and to which I turn in times of trouble.

Polya offers a strategy in four steps: (1) Understand the problem; (2) Devise a plan; (3) Carry out the plan; (4) Look back on your work

Those four steps are of course a bit trivial, but for each of the four steps he offers some extra advice.

- (1) *Understand the problem.* I often sit staring at a problem for a long time before I realize that I don't really know what I'm looking for. The first step is always to clarify what, exactly, you're trying to do. Polya lists some questions to help you get clarity on this. For instance, do you know what all the words or concepts in the problem statement mean? If not, go back to definitions. Try to draw a picture. Try to restate the problem in different words.
- (2) *Devise a plan.* Here, Polya offers a long list of ideas for how to tackle a problem. If you're stuck on a specific problem, try generalizing the problem. If you're stuck on a general problem, try solving a more specific version or a simpler version of a problem. If there are only a few possibilities, try all of them, one by one. If you have an idea of what the solution should look like, try to write that down and work your way backwards.
- (3) *Carry out the plan.* Try one strategy at a time. Take the plan you came up with in step 2 and try it out.



- (4) *Look back on your work.* If you solved the problem, perhaps the strategy you used could come in handy in the future and it's good to check again how you did it. If you didn't solve the problem, perhaps you can find the exact point where something goes wrong—some number turns out to be a constant instead of a variable, or the other way around—and this may give you inspiration for a new plan.

My favorite suggestions of Polya are to make a picture, to look at a special case, and to go back to definitions.

*Make a picture.* For one of my papers, I got stuck until I decided to turn to the eternal wisdom of Polya. It took me an entire evening to figure out how to draw the situation I was thinking about. But once I had found a way to draw it, the entire story fell into place. I could suddenly see how different cases in my paper looked graphically, and I could add a few extra situations which were obvious once I made a picture, but which I hadn't thought about while staring at the equations.

*Look at a special case.* At some point I was writing a paper where all kinds of things were happening at the same time—moral hazard, risk aversion, double marginalization. I kept getting stuck in a mathematical jungle until I used Polya's trick: look at a special case. By solving a case without risk aversion, and then a case without moral hazard, and then a case without double marginalization, I could see patterns emerge. If I then went back to the general problem I could see how the different pieces fit together. Every time you have a model with different effects working together, try to "turn off" one of them and see what the solution becomes and why. Do this for all the different pieces and it will be much easier to see the solution to the general case. The advice is not just specific to theoretical work. Also in empirical work, it helps to visualize (by making a scatterplot) and simplify (by doing simple OLS first).

*Go back to definitions.* If you have been staring at a problem for some time, maybe the problem is that you don't really understand what you are looking at. For instance, if you are studying cooperative game theory and you don't know how to demonstrate that some solution concept satisfies "individual monotonicity," maybe you should start by writing down the definition of that solution concept, and of that axiom. Quite often, just writing down the definition will give you a hint of where to look for the solution.

*Always Ask "How Much?"*

I'm a big fan of Deirdre McCloskey. Her works (in particular *The Rhetoric of Economics* [1998] and *Economical Writing* [1999]) should be compulsory reading for any PhD student in economics. In one brilliant essay, *The Secret Sins of Economics*, McCloskey (2002) explains that a good science should do two things: it should think and it should look. It should make theories about how the world works, and it should observe how the world functions to see if the theories are any good.

McCloskey's claim in the essay is that economics all too often pretends to do both, but in reality does neither. That is, she makes the claim that economics "engages in two activities, *qualitative theorems* and *statistical significance*, which *look* like theorizing and observing, and have (apparently) the same tough math and tough statistics that actual theorizing and actual observing would have. *But neither of them is what it claims to be.*"

McCloskey argues that theorists are too often satisfied when they have demonstrated an "existence" conclusion—that an equilibrium exists or does not exist; that something is Pareto efficient or not—without trying to assess the "how much" question. For instance, you could write a nice theory paper to show that in this or that situation, the market outcome is inefficient.

Interesting result—but how problematic is it? Has the pie become 2 percent smaller or 60 percent smaller? Surely, that makes a difference, and we would like to know.

The second secret sin is that much empirical work in economics is also not really asking “how much” but instead, like the theoretical work, seems to focus on *existence*, through a sad tradition known as “significance testing.” You look at the *t*-statistic or the *p*-value or the number of stars your statistical software puts next to the coefficient, and then you decide whether an effect matters or not. Economists sometimes forget that there is actually a coefficient to be interpreted too—sometimes we only look at the statistical significance and conclude that some effect exists or does not exist, while hardly talking about how big the effect is. There are so many problems with statistical significance I can’t even begin to discuss them all—I strongly recommend Ziliak and McCloskey (2008) for an introduction to the issues. For now, just consider the strange phenomenon that we sometimes calculate coefficients without really thinking about *how big* they are. Try paying attention to it next time you are attending a seminar or reading a paper, and you’ll see this happens more often than you would think. Whether you are looking at theory or data, asking “how much” is what economists should do.

*Feedback: Get It Early, Get It Often, Get a Lot of It*

I think this one is self-explanatory, but anyway: Your research group is full of smart people with more experience than you have, and you should make use of that opportunity. You don’t want to spend days or weeks working on some problem only to discover that your entire approach is wrong, or that someone else has done exactly the same thing already, or that you’re working on the wrong problem. More experienced people can point this out quite early. It may not always be nice to get criticism on your work, but it can save you lots of time down the road,

and I sure wish I had done this more often. Talk to colleagues, send drafts around, and give informal seminars to get some “friendly fire.”

### *Write*

Writing and research are not two separate activities. While you’re writing, you’ll discover that some of your arguments were not as solid as they seemed in your head; at the same time, putting thoughts down on paper will stimulate new thoughts. So, writing is an essential research skill. Of course, writing is also important because at some point you need to communicate your findings to others. Knowing how to write well is useful at that point. Again, writing well is a skill that can be learned—for instance by reading and using McCloskey’s *Economical Writing*.

### *Research Questions*

Now we come to a topic I’ve struggled with for several years. I don’t think I’ve found a good answer yet, but I hope these thoughts can help nonetheless. A good research question should satisfy a few criteria.

First, there should be a *market* for the paper you want to write. If you start from an existing literature and you’ve found an original way to contribute to a debate, that’s great. By contrast, you might have a nice idea but no clear sense of who would be interested in those results. That’s not good. Journals want to publish papers that will get cited. And, from a more philosophical point of view, you probably want to work on a topic that is important enough to attract attention of others. So, the first question to ask is: Is there a demand for this paper? It can save you a lot of stress if you know that you’re working on a topic other people care about.

Another way of putting this is that you ideally want your paper to be part of the “flow” of a conversation among scientists, where people pick up on each other’s arguments.<sup>9</sup> In a natural conversation, every contribution relates to what has been said earlier, and acknowledges what

other people said; in turn, your conversation partners will be interested in what you have to say if it's relevant to the discussion. You don't want to be the awkward kid who just blurts out something, then retreats into the shadows—your contribution would not really “link” to any conversation and would not be of interest to many people. This does not mean that you should never come up with original ideas, of course; but if you do, make sure you explain how they relate to previous discussions or why they are a worthy topic of conversation.

Second, you should have the *capabilities* of actually writing the paper. You can set yourself the goal of writing “The Paper That Explains Everything” (for which there would probably be a market), but that doesn't mean you can actually pull it off. In terms of capabilities, what matters is not what you can do right now, but what you can learn over time. If you're at the start of the PhD, everything may seem overwhelming; but if you pick a topic and focus on it, you can master the literature and the techniques, and gain the capabilities. So, assessing this aspect can be a bit tricky. Some questions to ask are:

- What kind of capabilities would you need to write this paper? Don't try to list *every* capability you would need (“literacy”) but focus on the most important ones. Would you need to know more about some statistical technique? Would you need to read up on an immense literature? Would you need to get your hands on a fancy dataset?
- Now that you made a list of the key capabilities, let's see what kind of capabilities you already have. Are you good at statistics? Do you already know the relevant papers? Do you already have a fancy dataset that would suit the purpose?
- For the capabilities you don't have, let's look around and see how easy it would be to obtain them. For instance, perhaps you don't know the relevant literature because you just started the PhD, but your research group is filled with people who know the field

inside out. In that case, it would be relatively easy to learn from your colleagues what the key papers are.

Third, you want to make sure the *competition* is not already doing exactly the same thing. You don't want to waste months writing a paper, only to discover that someone else already had the idea and beat you to it. A first check here, of course, is to do a quick literature search to see if someone else has a journal article or working paper on the topic you're interested in.

Fourth, you ideally want your paper to have *potential for follow-up work* for yourself. One possibility would be that your paper raises a bunch of questions which you can explore further. For instance, Nathan Nunn's original paper on the negative impact of the slave trades on economic development in Africa (Nunn 2008) led to a paper investigating the impact of slave trades on mistrust (Nunn and Wantchekon 2011) and a paper on how "rugged" landscapes affect this link (Nunn and Puga 2012). Another possibility is that your paper will teach you a lot about some analytical techniques (RCTs, ArcGIS, numerical methods, etc.) which you can use for other papers.

Ironically, I came up with this way of thinking about research while doing job interviews for consulting positions after I decided to leave academia. The checklist here is quite similar to a method sometimes used by companies when they think about launching a new product or enter a new market. Of course, it's much easier to write down a list of criteria than to actually find a research question. I can't offer more guidance at this point, mainly because I don't feel like I've mastered this area myself. Keep looking for tricks to find good research questions, and talk to people who seem to have an instinct for identifying good problems.

*Get out of the Comfort Zone*

During my PhD, I was fortunate enough to visit University of California–Davis. I can highly recommend such research visits. You can meet new people, discover new ideas, get feedback on your own work, and see first-hand how other environments are organized.<sup>10</sup> If possible, try to get an experience like this, whether it is doing fieldwork in developing countries (something I’ve unfortunately never been able to do), an internship in an international organization, or a research visit to another university.

### **Tricks for Productivity**

Finally, here are some tricks which could be useful either while studying or while doing research.

#### *Mind Your Energy Levels*

You are not a robot, and as a consequence you are not capable of working at full capacity for several hours a day and several days on end. Your mind is a part of your body, and just like the rest of the body it needs proper nutrition and proper rest.

Academia often fosters a macho culture where it looks cool to pull all-nighters fuelled by coffee or energy drinks and ignoring weekends. There’s no way that’s efficient. Your mind cannot function without sleep, without proper food, and without exercise. At the risk of sounding like your mother, make sure you get enough rest, make sure you eat your fruits and veggies, and go for a walk now and then. You’ll be ten times more productive afterwards.

One particular thing I discovered is that my energy levels can fluctuate a lot if I don’t eat regularly. For instance, if I eat lunch at 1:00 pm, I can feel my energy go down around 3:00 or 4:00 pm. The best strategy is to eat small things at regular intervals (a banana, say). This may sound totally ridiculous or trivial, but I’ve become a lot more productive since I discovered this.

Try to figure out for yourself how your energy levels vary during the day, and experiment to see how you can optimize them.

Also, take proper breaks. When I was an undergrad student, I would feel guilty about not studying, and so I would not allow myself to take a real break. Instead, I would sit at my desk staring at my notes for an hour. What a terrible waste of time! You're not studying, and you're not relaxing either. Relaxing is serious business; don't do it in a half-assed way. Go do something totally unrelated (take a walk in the park, go for drinks with friends, go watch a movie). You'll be more efficient afterwards, so there's no need to feel guilty, as long as you strike a good balance.

### *Avoid the Cognitive Switching Penalty*

Whenever your mind switches from one task to another, there is a switching cost. Your mind needs to figure out the context and all the relevant information for the new task, and this takes a bit of mental energy and time. As a consequence, if you're constantly switching from task A to task B and back, you are basically wasting energy and time—a phenomenon known as the “cognitive switching penalty.” Try to avoid this. If you're studying, turn off your cell phone and your e-mails. If you don't strictly need your computer for studying, shut it down. Find a way to study without distractions or interruptions, and focus on one thing at a time.

### *Don't Sweat the Small Stuff*

Think for a moment about all the tasks you have. Some will be urgent, others will be important, but those two groups of tasks do not necessarily coincide. Clearly, if a task is both important and urgent, you should tackle it. And if a task is not urgent and also not important, you can ignore it. But the other two cases are trickier. If you're not paying attention, it's easy to work on projects or tasks that are urgent, but not important.



One problem is that, psychologically speaking, it feels satisfying to check things off a to-do list. Most of the time, however, the important projects are vague and don't really fit well on a to-do list. This makes it tempting to spend your time on less important tasks. Moreover, once you're in the mindset of doing all kinds of urgent and small tasks, it can be difficult to calm down, concentrate and focus on big, possibly fuzzy, important projects.

One trick I've found useful is to start working on those projects first thing in the morning. For instance, when I need to figure out a theoretical model, I will first work on the model for some time before turning on my computer. The absolute worst thing I can do, is check my e-mails in the morning; that would destroy my concentration completely.

### **SOME CLOSING THOUGHTS**

My original idea was to write a few short pages, and I'm a bit shocked to discover this essay has turned out to be so long. I hope the length and the content don't scare you away! While the PhD can be very challenging, I have learned a great deal and met so many interesting people that the experience was definitely worth the trouble. By way of conclusion, let me quote from the essay of Ariel Rubinstein (2013), which appeared in a previous issue of this *Journal*:

Remember that you are one of the most privileged people on earth. Society has given you a wonderful opportunity. You are supposed to do whatever you want, to think about new ideas, to express your views freely, to do things in the way that you choose and on top you will be rewarded nicely. These privileges should not be taken for granted. We are extremely lucky—we owe something in return.

## NOTES

<sup>1</sup> And even as I'm writing this essay, part of my brain is telling me that I'm not a *real* economist and that some people in much tougher subfields of economics or much tougher universities would laugh at my pretensions, and so on, and so on.

<sup>2</sup> For more on this and other psychological phenomena, an interesting book is Myers (2000), in particular, page 299.

<sup>3</sup> For more on this, see Greene (2012).

<sup>4</sup> Of course, if you have serious and persistent motivation problems, perhaps you have chosen the wrong field after all. There's nothing wrong with that; the only question would then be how you want to deal with the situation. You could continue the PhD regardless, and find another job afterwards; or you could stop before finishing the PhD. This is not a failure in any way. It's impossible to know for sure if you will enjoy a certain job or not, and this is as true for academic jobs as it is for other jobs. Again, use the growth mindset here: You've learned more about yourself, your skills and preferences, and this knowledge will come in handy as you build your post-academic life. However, don't give up too fast. Like I said earlier, almost everyone I know has thought of quitting at some point.

<sup>5</sup> If you think this would also have some therapeutic benefit for you, the go-to reference is Gans and Shepherd (1994). Tons of classic novels have been rejected by publishers, too. Apparently George Orwell's *Animal Farm* once got rejected because "it is impossible to sell animal stories in the USA."

<sup>6</sup> Everyone understands the Pythagorean theorem, until you ask them to prove it. It's not that hard, really, but obviously we don't have the proof in our head when we use the theorem in a calculation. Yet, if you have a mathematical problem but suddenly you see how it can be

translated into rectangular triangles, a little light bulb will flash and you will “understand” the problem as being a variation on the Pythagorean theorem.

<sup>7</sup> Scott Young has called this “holistic learning.” See more here: <http://www.scotthyoung.com/blog/2007/03/25/how-to-ace-your-finals-without-studying/>

<sup>8</sup> See, for instance, the papers by Nathan Nunn on the long-run effects of the slave trade on African development (Nunn 2008; Nunn and Wantchekon 2011; Nunn and Puga 2012).

<sup>9</sup> This metaphor is attributed to Arjo Klamer (2007).

<sup>10</sup> Plus, it looks good on your resume!

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